THE UNPUBLISHED PAPERS OF J. J. LISTER.

A LARGE parcel of miscellaneous papers by J. J. Lister, and some pieces of apparatus, were left to the Royal Microscopical Society by the late Lord Lister. The papers were eventually submitted to me for examination, and this resulted in the discovery of the important paper on the limit of defining power in vision with the unassisted eye, the telescope, and the microscope now published in the society's journal.[1]

The underlying remarkable experiments were made, and a first—still existing—MS. prepared in 1831–2, shortly after Lister had published his famous paper on the improvement of the achromatic compound microscope (Phil. Trans., 1830, pp. 187–200). The manuscript was entirely rewritten, practically as now published, in 1842–3, and again revised in 1853; but although the author lived until 1863, he never published this excellent piece of work, which is interesting and instructive even now, fully eighty years after the doing of it and just fifty years after the author's death.

The original paper is now so easily accessible, and is so well worth reading, that instead of giving a detailed account of the contents I propose discussing more particularly those of Lister's results which are either of real interest even at the present time, or open to criticism as to their validity.

The most striking feature of the work—and undoubtedly so intended by the author—is its absolute homogeneity; for precisely the same objects were used in experimentally determining the limit of resolving power of the naked eye with full aperture and through circular apertures down to 0·00059 in. diameter, of telescopes of various apertures up to 4 in., and of a large number of good microscope objectives covering a wide range of angular apertures.

The objects, which are still in existence, being included among those now in the keeping of the Royal Microscopical Society, were very accurately made coarse gratings, produced by sticking paper on glass plates, cutting parallel lines through it, and removing strips at equal intervals. There was also a similarly produced chess-board pattern on glass, the separation from line to line or square to square being of the order of 1/10 in. This coarseness of the objects leads to the principal point worth discussing; for, as only 400 ft. distance was available, the objects could not be observed directly, even in the telescopic experiments, a diminished image of the actual object in a convex mirror having to be used instead.

In the microscopic observations a very greatly diminished image was employed which an auxiliary objective of higher power and wider angular aperture than that to be tested projected in the common focal plane of both. To most people this will appear as a perfectly legitimate proceeding involving only the most elementary optical assumptions and therefore not open to objection. But a crude method of carrying out this process with the microscope by observation of the small images formed by air-bubbles or fat-globules in a watery liquid has been severely criticised by Abbe for two reasons: first, because such images, when received by microscope objectives of wide aperture, must be affected to such an extent by aberrations as to render any calculation as to their size and nature utterly futile. Little can be said against this, but it does not apply to Lister, as he takes great care to state that the projecting lens was a perfectly corrected and carefully adjusted microscope objective. But Abbe raised a second objection of more serious aspect:—

> "Even supposing that a perfectly corrected projection-system be used, the observation is not really a microscopical one at all; it is a quasi-telescopic observation of the actual object by an instrument giving erect images, consisting of the projecting system and the real microscope, which latter acts merely the part of an erecting eyepiece."

There is no answer to this argument *so far as it goes*. But it really misses the crucial point, the only point of interest, altogether. And if we concentrate our attention on this, the question to be answered becomes this: Does the light received by the microscope from a perfectly corrected projecting lens differ in any essential respect from that which it would receive from a real object similar in every respect to the large one actually used, but diminished in size according to its distance from, and according to the focal length of, the projecting system?

The only answer to this question seems to me to be that there is no essential difference, and that Lister's results are perfectly valid. For on the older theory, which assumes that objects may be treated as if they were self-luminous, each point in the real object will send out spherical waves towards the projecting system, which turns them into perfectly spherical waves converging towards the conjugate point of the aërial image, from which they expand again so as to form, at a little distance, perfectly spherical waves *from* the same conjugate point as a centre, precisely as if the conjugate point itself were the true origin. The complicated interference phenomena which arise close to the focal plane, in what the late Dr. Johnstone-Stoney so aptly called the region of turmoil, have no effect on the form of the waves beyond that region. And if we adopt the Abbe theory we are led to substantially the same conclusion, for by the combined effects of the principle of equal optical paths between conjugate points and of the optical sine-condition we can easily show that the diffracted waves received by the projecting system are turned into such directions as to correspond exactly in every respect to those which would be sent out by an actual object of the size and structure of the ideal image of the real object.

We may indeed say that Lister not only gained the advantage of absolute homogeneity by his procedure, but that he avoided a very grave objection which, in fact, renders open to doubt,

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[1] Journ. R. Micr. Soc., 1913, pp. 27–55.

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if not absolutely futile, much of the work done by amateur theorists—namely, that observations for theoretical purposes must be made on objects of a structure which is perfectly known by some independent method, as otherwise we are moving in a "vicious circle." It is clear that in the case of the higher powers of the microscope this difficulty cannot be avoided with delicately marked natural objects examined directly.

The numerical results obtained by Lister are still of great value.

For the naked eye he finds that there is practically no gain in resolving power when the pupil is opened beyond 0·095 in.; that the difference in keenness of vision of different individuals rapidly decreases when smaller and smaller apertures are placed before the eye, and that with apertures below about 0·025 in. all reasonably normal eyes have the same resolving power which corresponds to his general formula. In this section we find a remarkable instance of the accuracy of his observations; for he records the fact that—contrary to his preconceived idea—two or three lines are more easily separated than a larger number; this has in recent times been theoretically confirmed!

The telescopic section is the shortest, owing to difficulties from unsteady air and bad light. But it includes interesting measurements of the diffraction rings of the spurious star-disc. The limit of resolution arrived at is 4·33 seconds of arc divided by the diameter of the object-glass in inches.

Probably the most remarkable results are those recorded in the microscopical section, which also contains the most surprising proof of the extraordinary accuracy of Lister's observations. For these enabled him to deduce that the resolving power of microscope objectives did not increase in proportion to the angle of aperture, but to the chord of that angle—in modern language, to what Abbe, forty years later, called numerical aperture. When it is considered that the widest angle accessible to Lister was one of 80°, and that even for that the ratio of chord to angle is merely that of 10 : 13, it will be realised that this was a very creditable feat.

The limit of resolution for the microscope arrived at by Lister, when translated into modern terms, is 95,240 lines per inch for N.A. 1·00. It should, however, be stated that although there are a very few observations reaching or slightly exceeding the figures finally adopted in the paper, these are not the *mean* of all the observations. Unlike some modern microscopists, Lister understood the meaning of the word limit as a practically unsurmountable barrier which should only be closely approximated under extremely favourable conditions, and he adopted his final numbers accordingly.

In conclusion I should like once more to advise those interested in instrumental optics to read this extremely interesting paper *in extenso*.

A. E. Conrady.

THE ANCIENT ARTISTS OF SOUTH-WESTERN EUROPE.

THE attention of readers of Nature has been directed from time to time to the numerous and interesting archæological discoveries in the caves and rock-shelters of central and southern France and northern Spain, which are due to the energy and skill of Prof. l'Abbé H. Breuil, MM. L. Capitan, E. Cartailhac, Prof. H. Obermaier, E. Piette, and others. During the last two or three years similar investigations have been made in other parts of Spain, mainly by the indefatigable Abbé Breuil with the co-operation of Juan Cabré Aguilo, Pascual Serrano Gomez, and Gomez Moreno, and we should like to take this opportunity of congratulating the Spanish archæologists on pursuing this fascinating line of inquiry, which already has led to important results.

The epoch-making monograph "La Caverne d'Altamira," by E. Cartailhac and H. Breuil (1906), is being followed by the publication of equally sumptuous memoirs on the more important French caves. These, together with the numerous papers and smaller memoirs that have already appeared (mainly in *l'Anthropologie*), prove that central and southern France and Spain north of the Cantabrian Mountains constituted an area throughout which the later stages of Palæolithic culture were spread with remarkable uniformity. This sequence consists of the Aurignacian, Solutrian, Magdalenian and Azilian industries, with their accompanying manifestations of glyptic and pictorial art.

The earliest phases of the art of the Franco-Cantabrian area are characterised by rude statuettes of the human figure, which are followed by beautifully executed carvings of animals in the round or in relief, and by large numbers of engravings on bone, ivory, and stone. The earliest engravings are linear scrawls, and even rude but vigorous drawings of animals on clay surfaces. The walls of numerous caves were also decorated with engravings and paintings of animals, of which the following sequence has been established.

First phase: the engravings consist of broad deep incisions; many of them are unrecognisable; some are profile representations of animals with but two legs drawn; the paintings have a similar character, but the earliest efforts were simple impressions in colour of the human hand and timid attempts at linear designs and grouped dots. Second phase: the incisions are still broad and deep, but the drawings are more lifelike; the four legs are shown, and hair is often indicated; the clever brush outlines of the earlier part of this phase are replaced by monochromes, some of which are beautifully shaded. Third phase: the engravings are now in thin lines; they vary in excellence, but some are real masterpieces; the paintings are deplorable, owing to an excessive use of pigment. Fourth phase: the engravings lose their importance, and in some cases are merely

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